a direct denial. There *is* a systematic arrangement in the whole book, or rather in the whole scheme embodied in both books. Because "H. E. A." fails to discover that plan which finds favour with him, it does not follow that systematic arrangement is absent. To say that the arrangement is not that which one would like to see adopted is fair criticism; but to imply that there is no alternative between one's own system and chaos is to expose one's own ignorance. And what is the feature of that system of practical chemistry in which alone the reviewer thinks the student can find salvation? He says, "in a properly chosen series of experiments everything should be proved; no assumption should be necessary." In another part of the review he tells us that "air and the phenomena of combustion should be first studied: the composition of air should be determined, and oxygen should be *discovered* by the student . . . The composition of water should next be qualitatively ascertained." I should be deeply indebted to "H. E. A." if he would kindly describe experiments on these subjects, suited to beginners in chemistry, in which no assumptions are made, and which convey sound teaching. He must not get over the difficulty by cleverly hiding the assumptions made, and so appearing to make none; everything must be proved, and proved by experiments which the beginner can satisfactorily conduct. I hold, and I am convinced that the history of science bears me out, that all scientific reasoning starts with certain assumptions, and that in every particular train of reasoning assumptions are made. If the beginner can be taught to recognize the assumptions which are involved in his reasoning on experimental data, he will do well. In the "Practical Chemistry" we have tried to emphasize the assumptions which the beginner must make. In our opinion the fatal thing is to cover over and hide away the assumptions; by doing this, the student acquires a habit of confounding hypotheses with facts, and so unconsciously he slides into loose methods of reasoning. I fancy I can detect the effects of such a method in the whole review: has not "H. E. A." tacitly, probably unconsciously, assumed that chemical truth abides with him and with him only?

We thank "H. E. A." for indicating some points in the descriptions of certain experiments which might be improved, and also for reminding us that the drawings of apparatus are not as good as they might be. These things can and will be improved. The mistake in the description of the diffusion-experiment, on p. 30 of the "Elementary Chemistry," to which "H. E. A." alludes, has been already pointed out to us, and a slip has been inserted in all copies except the first few hundred correcting this mistake. We cannot congratulate the reviewer, nor do we think he will be inclined on second thoughts to congratulate himself, on the trifling quibbles in which he has indulged regarding one of our experiments on the electrolysis of water.

Cambridge, January 23. M. M. PATTISON MUIR.

"Physical Science and the Woolwich Examinations."

I AM afraid that the moderation of your article on the regulations for admission to the military colleges may give some readers the impression that science is merely being discouraged more or less seriously in their examinations. The fact is, however, that it is being ousted with absolute certainty, for hardly anyone can afford to take up an optional subject which is at a disadvantage of 1000 marks. Severity of competition has within the last few years quite doubled the number of marks qualifying for admission to Sandhurst, and it will soon be impossible, even if it is not so at present, for a candidate to gain a place if he takes up any subject other than Latin, French, German, or mathematics.

This making all the men fit square holes whether they are round or not can hardly be for the advantage of the service, and one's curiosity is aroused as to the reason for such retrogressive changes—whether it is due, as has been asserted, to the action of head masters who do not desire to accumulate or encourage new-fashioned lore; or whether the military authorities really opine that to an officer who may have to deal with telegraphy, to choose a camping-ground, or perhaps direct a search for water, Latin is half as important again as electricity or physical geology.

Is it really too much to expect that they might insist first on a thorough knowledge of those parts of an ordinary education which are specially necessary or helpful to an officer, and then treat the unessential subjects on an equality as far as possible, and let a boy do in his preparation as he will when a man—adequately fulfil the duties of his position, and then follow his own bent? W. A.

January 30.

"The Art of Computation for the Purposes of Science."

HAVING read with much interest Mr. Sydney Lupton's second article on this subject, I think it right to draw his attention, and that of your readers, to Table III. of my book of five-figure and other logarithms published by Messrs. C. and E. Layton in 1870.

This table was framed by me for the purpose of enabling computers who occasionally require to use logarithms to ten places to get same with as little trouble as possible, and without shifting to any other book. In fact, I believe results can be got from my table almost as quickly as from the voluminous and beautiful volume of George Vega.

For instance, referring to Mr. Lupton's example, I find from my table and the instructions that log 1·0542482375 = log 1·05 + log 1·0040459405—this by simple division; then—

$$\begin{aligned} \text{By part A } \log 1{\cdot}05 \quad &= 0{\cdot}0211892991 \\ \text{By part B } \log 1{\cdot}0040459405 &= \underline{0{\cdot}0017535845} \\ \log 1{\cdot}0542482375 &= 0{\cdot}0229428836 \end{aligned}$$

correct by Mr. Lupton's solution from Vega.

My whole table is contained in eight octavo pages, and I believe is in as narrow a compass as is consistent with utility.

I may add that in the preliminary part of my book will be found a method of finding the logarithms of all numbers by nothing more than simple multiplication.

The late Prof. Augustus De Morgan, when I showed him this Table No. III., I well remember, replied: "It is very good indeed, but you will get no one to look at it," showing how rarely logarithms are ever required for any practical use beyond five, or at the most seven, figures. E. ERSKINE SCOTT.

6 Bond Court, Walbrook, London, E.C.,
January 18.

THE articles of Mr. Sydney Lupton on the above subject, which have appeared in recent numbers of your paper, do not profess to be complete; still, as their declared object is to assist those who are not mathematicians to work sums by the aid of tables, it seems to me that the best methods should not be passed over in silence, while others that are practically obsolete are discussed at length.

I beg of you therefore to allow me to call attention to the labours of the late Peter Gray, F.R.A.S., in the direction of supplying facilities for computing logarithms and antilogarithms. He contributed papers on the subject to various magazines; notably a series (with a table for formation of logarithms and antilogarithms to twelve places) to the Journal of the Institute of Actuaries in 1865. His most important work on this subject was, however, published as an independent volume in 1876. It is entitled "Tables for the Formation of Logarithms and Antilogarithms to Twenty-four or any less number of places"; and it contains, besides the tables, an explanatory introduction and an exhaustive historical preface. The published price is only 7*s.* 6*d.*, and it is therefore not beyond the reach of those who require such tools.

Weddle's method, the last mentioned by Mr. Lupton, consists in multiplying the given number down to unity, by means of a series of factors of the form $1 - (\cdot 1)_n \times r$, where r may take any integral value from 1 to 9. The logarithms of the factors are then obtained from a previously prepared table, and the complement of the sum of these logarithms is the logarithm of the given number. Weddle also used his method conversely, to calculate antilogarithms.

Hearn, of the Royal Military College, Sandhurst, improved upon Weddle's method, by substituting factors of the form $1 + (\cdot 1)^n \times r$ for the computation of antilogarithms, r, as before, ranging in value from 1 to 9; but he retained the factors $1 - (\cdot 1)^n \times r$ for computing logarithms.

Gray's improvements on Hearn were twofold. In the first place, he gave r the range from 1 to 999, taking for factors $1 + (\cdot 001)^n \times r$, and he thereby brought within narrow compass the arithmetical work involved. In the second place, by a simple arrangement of the calculations, he showed how to use factors of the form $1 + (\cdot 001)^n \times r$, instead of $1 - (\cdot 001)^n \times r$,

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for computing logarithms as well as antilogarithms; and thus, not only made the operations more convenient, but also caused one set of preparatory tables to he sufficient.

The principal table in Gray's book above-named consists of the logarithms to twenty-four places of all the possible factors $1 + (\cdot 001)^n \times r$, up to that limit. An auxiliary table contains, also to twenty-four places, the logarithms and their complements of the natural numbers 1 to 9, these being frequently required to "prepare" the given number. A smaller table to twelve figures only appeared, as already mentioned, in the Journal of the Institute of Actuaries, and was subsequently published separately by Messrs. C. and E. Layton; but as the twenty-four-figure table can be worked quite easily to any extent up to that limit, there is no particular advantage in the smaller one.

By means of Gray's tables the work of forming logarithms and antilogarithms is reduced to a minimum, and the process is so simple that any arithmetician can perform it, the more especially as many numerical examples are given in the introduction.

London, January 23. GEORGE KING.

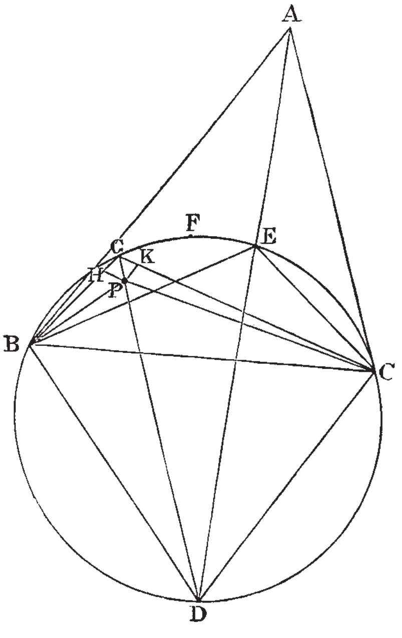
Note on a Problem in Maxima and Minima.

To find a point such that the sum of the straight lines joining it with the angular points of a given triangle shall be a minimum.

This problem was proposed by Fermat to Torricelli, who solved it, and sent it to Vincent Viviani, who also solved it, but called it a problem "qùod, ut vera fateor, non nisi iteratis oppugnationibus tunc nobis vincere datum fuit."

The solution is given in Gregory's "Examples of the Differential and Integral Calculus," and in Todhunter's "Differential Calculus," pp. 240–42.

Yet it can be solved in the most elementary manner.



Let ABC be the triangle. Describe an equilateral triangle on BC on the side remote from A. Describe a circle round the triangle BCD. Join AD. Then E is the point required. Join BE, CE.

(1) It follows, from Euc. vi. D, that

$$\mathrm{BE} + \mathrm{EC} = \mathrm{ED},$$
$$\therefore \mathrm{BE} + \mathrm{EC} + \mathrm{AE} = \mathrm{AD},$$

and evidently $\angle \mathrm{BEC} = \mathrm{BEA} = \mathrm{AEC} = 120°$.

(2) Let F be a point on the circumference BC.

$$\mathrm{BF} + \mathrm{FC} = \mathrm{FD} \quad (\text{Euc. vi. D}),$$
$$\therefore \mathrm{BF} + \mathrm{FC} + \mathrm{FA} = \mathrm{FD} + \mathrm{FA} > \mathrm{AD}.$$

(3) Let P be a point not on the circumference. Join DP, and produce it to the circumference at G. Let fall the perpendiculars PH and PK, on GB and GC respectively.

$$\text{By Euc. i. 26, } \mathrm{GH} = \mathrm{GK} = \tfrac{1}{2}\mathrm{GP}.$$
$$\text{Since } \angle \mathrm{GPH} = 30° = \mathrm{GPK},$$
$$\therefore \mathrm{BH} + \mathrm{KC} = \mathrm{PD},$$
$$\therefore \mathrm{BP} + \mathrm{PC} > \mathrm{PD},$$
$$\therefore \mathrm{BP} + \mathrm{PC} + \mathrm{PA} > \mathrm{PD} + \mathrm{PA} > \mathrm{AD}.$$

(4) It also follows from the above that if $\angle \mathrm{A} = 120°$, then the point required is $\mathrm{A} \equiv \mathrm{E}$.

If $\angle \mathrm{A} > 120°$, the point A will be within the circle, and A itself will be the point required. R. CHARTRES.

Note on the Dimensions and Meaning of J, usually called the Mechanical Equivalent of Heat.

THE title "mechanical equivalent of heat" tends to make one consider that J means the ratio of a quantity of mechanical energy to an equivalent quantity of heat; but since heat *is* mechanical energy (in a molecular form) it follows that J on this supposition is equal to *unity*, and therefore unnecessary.

Another way in which J is sometimes regarded is as the ratio between the ordinary *units* of heat and work; that is to say, in England, it is the ratio of the British thermal unit to a foot-pound, viz. the number 772. This definition makes it a simple number, the number of work units in a heat unit, a number which depends on the units of heat and work employed, and is different in France and England.

Now although J generally has one or other of these significations—that is, must be either unity or some pure number—yet people speak of the *dimensions* of J as being, not zero, but $\frac{\text{Work}}{\text{Mass} \times \text{Temperature}}$

It is evident that there must be some confusion here, a confusion arising from the fact that most people when *talking* of quantities *mean* only so many times the units of those quantities, and so are not always sufficiently careful about the definitions of the various quantities which they introduce.

Now if we confine our attention to quantities themselves, independently of any systems of measurement, we shall be led to a perfectly consistent mode of regarding J, a way moreover in which it will have the required dimensions $\frac{\text{Work}}{\text{Mass} \times \text{Temperature}}$.

A British thermal unit is the heat required to raise a pound of water at freezing-point through 1° F., and Joule discovered that the mechanical equivalent of that amount of heat was about 772 foot-pounds.

Hence if we wish to consider the work necessary to raise any other mass of water at freezing-point through any small range of temperature, we have only to notice that the quantity $\frac{\text{Work}}{\text{Mass} \times \text{Range of Temperature}}$ is constant, and equal to $\frac{772 \text{ foot-pounds}}{1 \text{ pound} \times 1° \text{ F.}}$.

This quantity is very fitly denoted by J, and might, if thought convenient, be called *a Joule*.

But this quantity is the *specific heat of water*, according to the definition that specific heat is the heat required to raise a mass through a small range of temperature *divided by* the mass and the range. So that we have arrived at these conclusions: a quantity of heat is the same thing, whether expressed in British thermal units, or in foot-pounds, or in terms of any other standard; and the specific heat of water at 0° C. is denoted by the letter J.

Indeed it may be said that the result of Joule's experiments is the determination of the specific heat of water in absolute measure. Again, if c is the ratio of the specific heat of any substance to that of water, the full expression of its specific heat is cJ; that is, its specific heat is some multiple or fraction of a *Joule*.

The first law of thermo-dynamics will then be expressed as—

$$dQ - p \,.\, dV = cJm \,.\, d\theta + m \,.\, dI,$$

where $dQ - pdV$ is the total energy supplied, $cJmd\theta$ is the amount of new energy evidenced by increase of temperature, and mdI is the increment of the latent energy of the body.

Coopers Hill, Staines, January 19. ALFRED LODGE.

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